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ON DIGESTION.

CONCOCTIO NON SINE ANIMA ET CALORE ABSOLVI POTEST.

THE means by which substances taken as food are converted into the elements of the animal fabric must have early attracted the attention of the curious. Various opinions have been advanced respecting them. At one period the digestion of the food was ascribed to trituration, at another to chemical solution; by some, putrefaction has been considered the chief cause, by others, fermentation. The opinion generally received on this subject at present is, that the food is converted into chyme by the agency of the gastric juice. It is the intention of this paper to consider the grounds upon which this doctrine rests; but in the execution of it, it is not designed to consider all the opinions that have been advanced on the subject of digestion. I have long regarded the doctrine of digestion by the solvent powers of the gastric juice as erroneous: I have in innumerable instances had occasion to observe and lament the pernicious effects that this doctrine has had on the treatment of disease,

particularly in that very common and distressing affection, dyspepsia; but my attention has been more particularly directed to it by a paper contained in the London Medical and Surgical Journal for June last.

“It is bad,” says the writer of this article, “to drink freely at or during dinner; as the gastric fluid will be too much diluted, and rendered unfit for dissolving the food in the stomach.”

This doctrine, proceeding as an editorial article in a well conducted and deservedly extensively circulated journal, cannot fail, if allowed to pass unnoticed, to give additional weight to opinions which I think may easily be proved to be erroneous, and to confirm practitioners and the public generally in a treatment which I deem most mischievous.

Gastric juice is the name given to the liquid which is secreted from the internal surface of the stomach. It is a bland insipid liquid, neither generally acid nor alkaline, inclining to acid in the herbivorous animals. As a secretion, it is scanty; in point of quantity of liquid, not more than would be required for the lubrication of the organ.

The experiments and observations upon which the doctrine that the food is digested by the solvent powers of the gastric juice rests, are those chiefly which were made by Reaumur, the Abbé Spallanzani, and Mr. John Hunter.

The experiments of Reaumur related to two objects. The first was to ascertain the effects of the gastric juice in the stomachs of living animals; and the

second, to ascertain those effects on the substances usually taken as food out of the stomach, or rather in vessels unconnected with any living substance. In the experiments which were used for proving the influence which the gastric juice might have upon substances introduced into the stomach of living animals, contrivances were judiciously and effectually made for allowing the secretions of the stomach to have free access to the food, which at the same time was protected from any effects which friction might have upon it by the movements of the stomach. In these experiments the food was found to be dissolved or digested fully in the time that is usually required for that purpose, when taken into the stomach, without any protecting vehicle, and allowed to move freely in that organ.

In order to ascertain the effects of the gastric juice upon the same food out of the stomach, but at a temperature equal to that of the living animal, he procured a quantity of gastric juice, by contrivances which appear to have been judiciously made, and efficient for the purpose in view. With the fluid thus procured he mixed a quantity of flesh or grain bruised. The proportion of fluid used was much greater than can at any time exist in ordinary digestion. A few grains of flesh were introduced into a phial containing two drams of gastric juice. The heat was maintained at least equal to that of the living body. At the end of nine hours, the fibrous structure of the flesh still remained entire.

Little or no alteration was observed to have taken place in it. After repeating these experiments, varied with great skill, and evidently with the bias of an expectation that the gastric juice would in these circumstances dissolve the food as fully as in the living body, he was brought to the conclusion that the gastric juice has no more effects out of the living body in dissolving or digesting the food, than water mucilage, milk, or any other bland fluid.

At this point the inquiry respecting the power which the gastric juice had in digesting the food ought to have terminated in the rejection of such a power; for whatever effects might be produced upon the food in the living stomach in conjunction with this juice ought fairly to have been ascribed to the additional properties which it obtained in that condition.

The Abbé Spallanzani multiplied and varied the experiments of Reaumur. Substances introduced into the stomachs of living animals, after the manner that had been done by Reaumur, were digested. Spallanzani's attempts to dissolve flesh or bruised grains out of the body, by means of gastric juice, were scarcely more favourable to the doctrine of the solvent property of the gastric juice than those of Reaumur. The most complete approximation to digestion was made in the following way:—A small quantity, five grains, of flesh was introduced into a phial containing two ounces of gastric juice exposed to the heat of the sun, raising the contents of the vial to 23° of Fahrenheit's scale. At the end of nine hours

this flesh had become soft and pulpy externally. When introduced into a fresh bottle of gastric juice, at the end of six hours more, the fibrous structure had entirely disappeared. In this instance, fifteen hours, at a very high temperature comparatively, with an exposure to a strong light, and a renewal of gastric juice, were required to accomplish what is done in the dark recesses of the stomach in four hours, at a much lower temperature. It is quite supposeable that the same effect would, in these circumstances, have been produced upon the flesh had water been the menstruum.

In some of the experiments, in order to give the flesh or grains steeped in the gastric juice the same temperature with the body, the phials were introduced under the armpits. But this is not a fair mode of ascertaining the effects of the gastric juice out of the body. For the influence which life may be supposed to have on the solution of the food would be secured in this case. The affinities connected with life would extend to substances in contact with any part of the system: substances placed under the armpits, are not placed, at least, in the same circumstances with those entirely unconnected with a living animal.

The experiments of Reaumur and Spallanzani give no evidence that the gastric juice has any peculiar influence more than water or any other bland fluid in digesting the food.

It may be worthy of remark, that the most dis-

tinguished scientific physiologists who flourished at the time, and were well acquainted with the experiments of Reaumur and Spallanzani, refused their assent to the doctrine of the solvent properties of the gastric juice. Haller, of Germany, and Thenard, professor of chemistry in Paris, maintained that these experiments gave no support to the doctrine. But it is chiefly owing to the observations and reasoning of Mr. John Hunter that the doctrine of the solvent property of gastric juice became prevalent. This gentleman found, on dissecting the bodies of some persons who had been cut off by violent or sudden death, that the stomach was occasionally holed at its large end opposite to the spleen; that the edges of the holes had the appearance of being half dissolved or digested; that this destruction was not confined at all times to the stomach, but that it sometimes extended to the spleen, diaphragm, and lungs, a great portion of which viscera were consumed; that the stomach at every other part, excepting for a small distance around the edges of the holes, was perfectly sound; and that the contents of the stomach, which appear in these cases to have been always considerable, were found to have passed into the cavity of the belly.

From these appearances Mr. Hunter concluded that the stomach was digested or dissolved by the gastric juice which was contained in the stomach at the time of death, or which was still contained in the secreting vessels, and not yet poured into the

cavity of the stomach ; that as the cause why the stomach is not digested by the powers which dissolve the food it contains, is the preservation of this organ by the principle of life resisting putrefaction ; the stomach, by death, is deprived of this conservative principle, and then, like the food it contains, yields to the dissolving powers of the gastric juice. Hence was deduced, what was by him, and still continues to be held by many, a convincing proof of the solvent properties of the gastric juice, and that by it alone the food was dissolved in the stomach.

So many and great objections to this doctrine arise at first view, that it seems surprising it should have been entertained for a moment. The great authority of the proposer of it must have given it a weight and currency to which it was not entitled by its merits. If the holes in the stomach discovered by Mr. Hunter had been produced by the gastric juice, the whole internal surface of the stomach would have been affected. No reason has been advanced why the solvent power was confined to a particular part of the organ. Indeed, the portion of the stomach most generally dissolved would not be that, as will afterwards be shown, to which the gastric juice, in the circumstances stated, would have readiest access. The extent of destruction was occasionally far greater than could reasonably be supposed to be produced by the gastric juice contained at the moment of death in the stomach. A great portion of the spleen, diaphragm, and lungs was dissolved in some cases.

The quantity of gastric juice secreted during the digestion of a meal (the time at which it is supposed to be done in the greatest abundance,) does not, in the human body, exceed an ounce or two, according to a great authority, Dr. Fordyce. We have seen how tardy and slight were the effects which this quantity had upon a minute portion of flesh. How could it be supposed to have so complete an effect upon such a mass of material? It is not certainly supposed that the gastric juice is secreted after death. If the effect were chiefly produced by the gastric fluid, though formed, still contained within the secreting vessels of the stomach, as seems to have been Mr. Hunter's opinion, and enabled to act in that condition on the vessels that had just secreted it by their being deprived of the protecting principle of life, the whole stomach would have been equally affected; for there is no reason for supposing that the secretion of the fluid is not performed throughout the whole stomach, far less for supposing that it is confined to the particular part of it that was found to be consumed.

A great part of the argument in favour of the solvent properties of the gastric fluid is founded upon the doctrine that the living system is prevented from undergoing that change called putrefaction which commences in the materials of which it is composed soon after death, by the principle of life. This doctrine I believe to be without foundation; and I have attempted, and I believe successfully, to show,

in the article on "Absorption," contained in a physiological work recently published, and more extensively in an article on the "Circulation of the Blood through the Capillary Vessels," that the process of putrefaction goes on as rapidly in the same circumstances during life as it does after its termination, but that the effects of it are prevented from being perceived by the removal of the products of it before they become offensive to the senses ; by their instantaneous conversion into salutary fluids ; and by the repair which contemporaneously takes place.

So long ago as the year 1808 I attempted to explain the cause of the holes found in the stomachs of persons cut off suddenly, in a pamphlet which I had occasion to publish at that time ; and I am still disposed to believe that the account which I then gave of them is substantially true. These holes I considered to be the effect of putrefaction, directed by circumstances to a particular portion of the stomach and adjoining organs. In persons in whose stomachs the holes were found upon dissection, the deaths had all been sudden. In some cases it has happened that the death had been preceded by lingering disease, but nevertheless the death was sudden and unexpected. The death took place while the heat of the body was as high as, or higher than, the temperature of health. The reason is, that the high temperature favourable to putrefaction remains with the corpse longer than it does after death produced slowly and gradually. Another circumstance

particularly deserving of remark, in the cases in which the holes were found, was the fulness of the stomach with materials that were slow conductors of heat,—the high temperature thus being continued longer in the stomach than in the rest of the body. That temperature would be continued longest without diminution at the top of the column of liquids filling the stomach. For while the particles of liquid at the top of the stomach became cooler, they would in consequence of their greater gravity descend, and their place would be occupied by the lighter and warmer liquid below. The part of the stomach adjacent to, or in contact with, the top of the column, would be kept longest in a state favourable to the putrefactive process. But this is the part at which, when the body is laid out after death, the holes of the stomach are found. The adjacent organs, spleen, diaphragm, and part of the lungs, would be placed in the same situation with the superior parts of the stomach. This explains why one part of the stomach is holed or destroyed, while the rest of the organ is sound. It may be contended, that if the holes of the stomach were the effects of putrefaction, an offensive smell would be noticeable, which is not the case. But it must be remembered, that the stomach in all cases contained a quantity of vegetable matter. It appears, from the experiments of Sir John Pringle, that though in such circumstances the putrefactive process may have advanced far, the vegetable fermentation, succeeding rapidly, overpowers or neutra-

lises the putrefactive effluvia, and even produces a sour smell.

Holes similar to those described by Mr. Hunter have been found upon dissection in other parts of the alimentary canal. They have been found in the large intestines after sudden death, when the bowels were much distended with fluid. In these cases the holes could not be occasioned by the gastric juice.

These holes appear to be chiefly the result of the putrefactive or fermentative process. There exists besides, in all animals, an unseen agent of extensive and powerful influence. It has been called the principle of life. It would appear to be some modification of the electric fluid. It presides over, and regulates the renovation of the fabric. It may be conceived, in cases of sudden death, and while the heat of the system remains, to adhere for some time to the dead fabric. That galvanic excitability adheres strongly for a considerable time, after the destruction of life by violent means, was awfully proved some years ago by the contortions and life-like gestures and movements that were made by the body of a criminal newly executed at Glasgow. The destructive part of its renovating powers may continue, while the restorative or repairing part may cease to have effect. The destructive part of the process of renovation may easily be supposed to proceed, while the restorative shall have ceased; for the latter, to have effect, requires a supply which is necessarily withheld by the inaction or destruction

of the vehicles, in which only it can be conveyed. Analogous processes are not without example in the animal machine. At death, and sometimes before it, the powers depending upon irritability cease, while those depending upon elasticity, an agent unconnected with life, continue their operation. Hence the accumulation of blood in the chest, and its desertion of the arteries and smaller veins, at the instant of the most sudden death, and before that period often producing the *facies hippocratica*, the sure prognostic of approaching dissolution.

From the preceding statement we are fully warranted in concluding, that out of the body the gastric juice has no power of dissolving the substances usually received as food, further than supplying them with liquidity; that no proof has been supplied that it is more essential to the conversion of food into chyme, than water or mucilage, or any other bland liquid; and that, considering the smallness of the quantity supplied during the digestion of a meal, and the blandness of its quality, the presumption is that the food cannot be digested by this menstruum. The quantity of gastric juice secreted does not exceed what, as a liquid, would be required for the lubrication of the stomach. The presumption is that that is the purpose for which it is produced.

We come now to supply, what seems only to be wanted for completely subverting the hypothesis of the digestion of the food by the gastric juice, the evidence of the system being possessed of the power

of digesting the food in the usual way in which it is accomplished in the stomach without the aid of the gastric juice.

This evidence is fully supplied by a valuable series of experiments, which, about twelve years ago, were made by Dr. Sillar and Dr. Hood, in Liverpool, and published in the *London Medical and Surgical Journal* for 1822, and subsequently reprinted in a work published in Liverpool by Dr. Hood, entitled *Analytical Physiology*. These experiments are not only valuable as supplying a foundation for the subversion of a doctrine which has long been mischievously predominant, but by them a property appertaining to living matter, of great importance, has been clearly exhibited, and conclusively ascertained. The experiments for ascertaining the effects of the gastric juice out of the body, made by these gentlemen, do not differ in their results from those of Reaumur and Spallanzani, and need not now be noticed. The original and most important part of the experimenting was made with the view of ascertaining how far digestion might be accomplished in parts of the living body to which the gastric juice could have no access. For the purpose stated, they made an incision into the thigh of a dog, and introduced into the wound a piece of flesh, and retained the flesh in the wound by stitches. At the distance of nine hours, the flesh was withdrawn, and examined. They found that an adhesion had taken place between the flesh and the sides of the wound.

Excepting at the part at which the adhesion had taken place, and at which the flesh had actually become a part of the living animal, it was reduced to a soft saponaceous mass. Into the same wound a slice of mutton was introduced, and secured, as in the former instance, in its place by stitches. They intended to have allowed twenty-four hours to have expired before making the examination of the state of the mutton, but at the distance of seven hours, observing a slimy matter to issue from the wound, they were induced to examine it immediately. They found the mutton reduced completely to a soft saponaceous mass, without the existence of fibrous structure. Here then is the digestion of a piece of food as completely as it is ever done in the stomach, nearly in as short a time as it ever is supposed to be performed in that organ, without certainly any assistance from gastric juice. Had it been examined sooner, the digestion might have been found to have been completed in a shorter time. Other experiments were made of the same kind, and with the same results. One circumstance which cannot fail to expedite digestion in the stomach was wanting in the pouch of the thigh. This is the agitation which the contents of the stomach are constantly undergoing by the peristaltic motion of the stomach, and more extensively by the process of respiration producing a regular succession of movements in all the abdominal viscera. By the application of fresh influences, the solution must be necessarily hastened. Taking this

circumstance into consideration, the digestion must be admitted to have been as complete and quick in the pouch made in the thigh of the dog as it could have been in his stomach.

There are some circumstances which are noticed to have happened in the experiments of these gentlemen that have led them to ascribe a slight solvent effect to the gastric juice, more than to water, or any other bland diluent of the same temperature, but which I think ought not to have done so. A piece of meat was introduced into a phial containing gastric juice, and retained in the rectum of a dog. One end of the meat was bound by a string for the purpose of drawing it easily out of the phial. The part tied by the string was not so completely digested as the rest. But was not this to be ascribed rather to the compression not allowing the parts tied to separate, and mechanically preventing a change of structure, than, as was supposed, to the want of access of the gastric juice? A piece of mutton, immersed in gastric juice, was deprived of its fibrous texture, in circumstances in which a piece of the same substance, mixed with saliva, was not so affected. But this, as was suspected by the experimenters themselves, was the effect of the great viscosity of the saliva. The food, mixed with saliva of this description, was prevented from solution by the same cause by which articles of diet are preserved in vessels containing above these articles a layer of oil or fat.

In some of the experiments made for ascertaining

the effect of the gastric juice out of the body, in order to give to the food and the juice mixed with it the same temperature as the body in health, the phial containing them was introduced into, and retained in, the rectum of a dog. In other experiments, the phial containing the subjects of experiment was kept inside the armpit. But the results of these experiments are not to be taken as the effects of the gastric juice out of the body. The anima or principle of life, supposed to be of a galvanic nature, and which has a great effect upon the digestion of the food, must have reached the contents of the phial so situate as fully as if their contents had been placed inclosed in the stomach.

So wholly visionary and baseless is the foundation of a doctrine which has spread its baneful influence over the habitable world, and bound its votaries, in every country into which it has been admitted, as with a spell. It is the basis on which the rules laid down for the preservation of health, in the works lately published on digestion, particularly in London, which was the birthplace, and still continues the stronghold, of this doctrine, are founded; and the numerous editions which have been required of many of these works prove the extent and influence of its dominion. But I maintain that it is the parent of disease, both mental and corporeal, in a thousand melancholy forms. It is a monster with a hundred arms, each the instrument of destruction. In its train are found dyspepsia, spinal irritation, hysteria,

hypocondriasis, intemperance, despair, and suicide. It is the parent of burning with moxas, of excoriating with cantharides and antimony, of setons, issues, and all the torments of the contra-irritant scheme. It feeds upon pills, powders, upon restoratives of every clime, and upon purgatives of every power. It stores abundantly the pockets of those over-busy, ever-something-to-be-doing-practitioners, whose last visit never fails to secure a reason and apology for another.

Having for many years been convinced that the gastric juice did not act upon the food as a chemical solvent, and that digestion was not impeded by the free dilution of it, I have directed the drinking of water freely at dinner, and at every meal where solid food is used, as a general rule for the preservation of health, and in the treatment of dyspeptic, and what are called nervous diseases; and so far as I can judge, with marked success. My directions in this respect have become so remarkable in the place in which I practise, that my prescriptions have jocularly been attempted to be anticipated by the friends of the patients, who have expressed their intention of consulting me, and silyly attempted to be ridiculed by others with views not quite so innocent. The time seemed therefore to have arrived that I should unfold, publicly and fully, the reasons of my practice.

It is a singular circumstance that, in fashionable life, in which the doctrine that I have attempted to refute chiefly prevails, wine does not appear to be one of the liquids by the mixture of which the

powers of the gastric juice can be diminished. This has arisen perhaps from a less quantity of liquid of this description being required to satisfy thirst, or from a supposition that its use may add to the powers of the gastric juice, or more probably from a supposition that a stimulant promotes the secretion of the fluid by the stomach. Hence arises the prevalent practice of drinking wine, and that liquid only, at dinner. In my early days it was deemed indelicate, or a violation of politeness, to ask a lady to drink wine at dinner, into whose glass any of that liquid had already been poured. But now, every gentleman must drink wine with every lady at table. This practice is in many respects bad.* In the first place, it prevents the drinking of water or buttermilk, far more wholesome diluents, for neither of these liquids will go down after wine has been tasted. They are too insipid for the wine-vitiated palate. Thirst urges drinking at the time, and after every glass the fear of drenching the gastric juice lessens. An opportunity is thus afforded of taking more wine than would otherwise have been decent in females; and young ladies, from inadvertency or from a desire to please, may be induced to take more than is for a time agreeable to them, or than is due to the proper protection of the habits. Hence the practice of indulging in wine is formed. I constantly urge that water alone should be drank during dinner, and that wine, if it must be taken, be

* Although the fashion of the present day is changed, still wine is used very extensively at dinner, and what is worse, mixtures of wine are now more taken than when the above pamphlet was written.

deferred till after the meal is finished. The thirst is satisfied with the bland liquid; the desire for stronger is abated or altogether destroyed. I have almost always found that they who can be prevailed upon to drink water freely for some time at dinner, will be disposed to drink nothing else. Every other liquid will be felt by them to be too gross as a diluent to the food. It becomes grateful to them in the highest degree. The doctrine that water should not be drunk during dinner tends indirectly, but powerfully, to promote intemperance.

If water is so useful a beverage at dinner, how happens it that many people enjoy good health who never drink water or any other liquid at dinner, or very little of anything at any other time? In answer to this question, it is to be observed, that water or any other liquid is not necessary to the digestion of the food; it is only contended that digestion is not impeded by the quantity of liquid that is taken during meals, or immediately after them. Solid food during mastication is mixed with saliva in the mouth; it is further diluted in the stomach by the juices secreted by that organ. So far as digestion actually is concerned, that dilution is sufficient. But liquids are required more abundantly for other functions. All the membranes and ligaments of the body require to be kept in a constant state of lubrication; secretions are to be fed; the waste of liquid by perspiration and lubrication is to be replaced. When we observe how little some people do drink, and that

little is often of so heating and stimulating a nature, apparently incompatible with the purposes which liquids are required to serve in the system, we are surprised that health could be preserved for a day, instead of its being so for years uninterrupted, under such circumstances. If in such cases there existed no other source from which liquids are received into the system than that afforded by the mouth, the fabric would soon be broken. But there is another source from which the system is plentifully supplied with liquid, and which has only been satisfactorily explained within a short period. Dr. Edwards, of Paris, in a valuable work on the influence of physical agents on life, has proved that the air we inspire, without undergoing any change, enters directly into the mass of blood. The same doctrine, reached by a different mode of investigation than that pursued by Dr. Edwards, was maintained by myself, in a paper on the "Sources of Animal Heat," that was originally read, many years ago, before the Literary and Philosophical Society of Liverpool, and afterwards published. According to this doctrine, the air we breathe, uniting directly with the blood in the pulmonary veins, is conveyed to the heart, and circulated by this organ through the whole of the body. The greater portion of the air thus introduced is converted from the aërial into the liquid form, partly by chemical, and partly by mechanical combination with the blood. The atmospheric air has at all times suspended in it a quantity of water. This is also

introduced into the mass of blood. By the process of breathing an accession of liquid is constantly made to the system. This explains why an hydraulic machine, which the human body may be considered to be, is kept in action without any considerable supply, in some cases, by the mouth. The supply required by the mouth may be greater in some constitutions than in others, and in the same constitution at different times. This may explain the different propensities of different persons for drinking. How then is the varying supply required to be regulated? Nature generally becomes the monitor when the supply ought to be increased by exciting thirst.

In some morbid cases of the system this monitor does not interfere, though particularly required. Many persons affected by dyspepsia are never thirsty, and have an aversion to drinking, though they are afflicted with continual pains in the stomach, annoyed with retching and flatulency, and dejected by the effects of constipation. The mouth in such cases is frequently filled with water; there is a constant water rash. It would appear that the throat and fauces are moistened by some morbid secretion, while other parts are, as it were, parched. If such persons can be prevailed upon to drink plentifully of water, their cure is certain. It is in such cases that mineral waters afford such surprising relief. Dryness of the skin, constipation of the bowels, high coloured and scanty urine, where no organic affection is suspected, are the sure indications of deficient dilution. No danger is

to be apprehended on the side of taking too much mild liquid. For removing any redundancy of that nature the system is possessed with ample means.

It may be asked, What other cause can be assigned for digesting the food except the gastric juice? By what other agency can imperfectly masticated pieces of mutton and bread be reduced to chyme, the product of digestion on these substances in a few hours? It may be replied, that our inability to assign any other satisfactory cause for the digestion of food would not in the slightest degree strengthen the claims that may be urged in favour of the solvent powers of the gastric juice for that purpose. These claims are for ever excluded by positive proof.

Digestion appears to proceed on the groundwork of animal and vegetable fermentation.

There exist in the stomach of a living animal all the influences which are most favourable to this process. Heat, moisture, and confined air are, according to Sir John Pringle, the conditions most favourable to putrefaction. These exist in the stomach. To these may be added, as powerfully co-operating, the agitation, or, if it may be so called, churning, which is constantly given to the contents of the stomach by the process of respiration. But in addition there must be some other influence. This I conceive to be what, in the motto I have selected from the works of Aristotle, is called *Anima*. What this *anima* is may admit of dispute. That some modification of electricity always pervades the living

fabric there can be no doubt. The contraction of the muscles is known to be produced through an electrical or galvanic agency. This powerful chemical agent, therefore, pervades the system at all times. The efficiency of the agent which overcomes the strongest affinities which are found to exist in nature may be admitted to be adequate to the solution of the food, with the aid of the agents stated. But digestion does not consist of solution alone. The products of solution are combined by it into a uniform mass nearly of the same qualities, whatever be the substances from which it is formed. This is called chyme. Upon such a resemblance the directions for selection of food are frequently founded. Those articles which approach nearest to chyme in their appearance and qualities appear to be of the most easy digestion. The labours of the stomach are supposed to be already half performed. But this is erroneous. The powers of digestion and assimilation of the stomach do not most readily and easily convert into the substance of the system those articles of diet which most nearly resemble that substance. Milk bears a strong resemblance to chyle, that completely assimilated liquid which is absorbed from the bowels and converted directly into blood; but milk must be reduced into its elements, and recombined, before it form any part of the chyle it so nearly resembles. Digestion proceeds from causes too powerful and uniform to be affected by the minor distinctions of diet. Minute directions respecting the articles of

diet, prohibiting one article and admitting another, under various endless modifications and exceptions, may usefully impress the mind of the patient with the reverence due to unequalled knowledge and unerring skill, and secure the homage such high qualifications merit; they may abundantly answer the purposes of the Spa practitioner; and may give a rapid and boundless circulation to the pages of the dietetic writer, so greedily devoured by the valetudinarian, who, in consequence of the knowledge thence derived, swallows every mouthful with fear, and in whom every uneasy and new sensation rouses the apprehension of having been poisoned by some prohibited article, incautiously and inadvertently taken at the last meal; but they are unnecessary, and they are dishonest—Nature does not require their aid.

The same powerful controlling anima, which operates in the solution, modifies the combination. The cause assigned by Aristotle for the digestion of the food seems to want one ingredient—that is, humidity. If the sentence had run thus: *Concoctio non sine anima, calore, et aqua absolvi potest*, the aphorism would, in my opinion, have been complete.

ON THE POWERS GENERATED AT THE EXTREMITIES OF THE
CAPILLARY VESSELS FOR MAINTAINING AND PROMOTING
THE CIRCULATION OF THE BLOOD.

It is maintained in an article on absorption, published in a recent work of mine, that the veins absorb and carry off all the recrementitious part of the system, while it was the exclusive office of the lacteals, lymphatics, and imbibers of the lungs to supply the nutriment or what was required for the growth or repair of the body. The recrementitious matter, it was contended, entered the venous extremities at the moment at which these extremities were receiving the part of the arterial blood not expended in the repair of the system, or in the formation of secretions.

The manner in which the communication necessarily allowed to exist, by which the blood flowing from the arteries reaches the veins, is made, has, as was observed in that article, been a matter of controversy; some contending for an immediate communication, while others argue in favour of the existence, in all cases, of such intermediate receptacles as are admitted to exist in some parts of the body. The

nature of the communication alluded to is not generally the object of observation. The mechanism employed has not been subjected to the examination of the senses by any contrivance hitherto discovered. We are, therefore, obliged to have recourse to other expedients for the discovery of the nature of the communication. Not being able to observe what does in reality take place, we are obliged to ascertain, if possible, by an induction of facts, what must of necessity take place. Nor is this method of investigating the works of nature less satisfactory often in its results than the conclusions derived from actual observation.

It was contended in the article alluded to, and (as has been just observed) that the materials destined for the nourishment of the system were absorbed by the lacteals, lymphatics, and imbibers of the lungs, and ultimately conveyed by these vessels into the arterial current; and that the refuse of the solid or fixed parts of the body, as well as the refuse of the blood, was carried off by the veins. In the present inquiry, therefore, we are limited to two sets of vessels—the arteries by which the nutriment is deposited and the veins by which the recrement is withdrawn.

It is on the supposed truth of the doctrine respecting the share which the veins have in the office of absorption, as contended for in the article to which I have repeatedly alluded, that the following argument proceeds. The present paper, therefore, is to be considered as an addition to that article; and

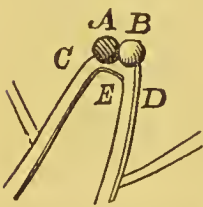
it is desired that they who may honour that article with a perusal will introduce this paper after page 200 of the work "On the Causes of Respiration, of the Motion of the Blood, of Animal Heat, Absorption, and Muscular Motion;" or that previous to the perusal of this paper, the article on absorption will be studied.

An important part of the office of the arteries is the supplying of the fresh materials for the repair of the system. This and their office of conveying blood to the terminations of the veins are the objects of our present attention, so far as the arteries are concerned; and, with respect to the venous system, the power of absorbing and withdrawing the recrement of the body, and of receiving the blood transmitted to their extreme orifices by the arteries. When a particle is added to the solid or fixed parts of the system by the arteries, the orifice of the arterial capillary depositing this particle must be in contact with it. Another particle, which had for a period constituted a part of the solids, is in all probability displaced by it; and being relieved from its adhesion falls into the sphere of venous absorption. The cavity of the venous capillary or channel receiving the particle must have had a communication with it before its separation. What is now stated with respect to single particles must equally apply to all the particles composing the solid parts of the body. Therefore all the solid parts are reached by and contained in the vascular.

But, besides the particles deposited for the repair of the system, there are others, and I suppose by

far the most numerous, which are transmitted from the arteries, and which, without any location, are received by the veins. The orifice of an arterial capillary has, therefore, two communications : one with the particle of nutriment which has been located ; another with the corresponding venous capillary, either mediate or immediate. The cavity of the venous capillary has also, through its extreme orifice, two communications : one with the particle of solid which is in the process of detachment from that solid and ready to be absorbed, and another with the arterial orifice either directly or through the intervention of cells.

This mechanism may be illustrated by a diagram :—



Let A represent a particle of fresh material supplied to a bone or muscle from the arterial capillary C ; and B the particle of recrement, the place of which is to be supplied by it, and D the venous capillary into which the venous particle B is passing, and E the passage through which the unlocated blood flows from the artery into the vein.

There may be other outlets, porous or organic, from the channel E forming the passage through which the blood passes from the arterial orifice C to the venous capillary D ; but with these our present argument is not concerned. The condition and progress of the displaced or recrementitious particle B demands our particular attention. I have endeavored to prove, in another work, that it is this particle which,

mingling with the blood, changes the colour of that fluid from the vermillion to the purple, or which converts it from arterial into venous.* What is the cause which has made this particle be separate from its adhesion? Perhaps this question cannot be exactly answered. There cannot be a doubt, however, that it is allied to that power which produces fermentation in animal substances. Consequent upon the separation of the particle B from the substance with which it had been in a state of cohesion, the centre of the particle B will be farther removed from the centre of the particles of matter to which it was con-

* It may be proper to take notice of an experiment of Bichat's, which appears to that eminent writer to prove that the colour of the blood is changed from the purple to the vermillion in the lungs, by the accession of atmospherical air. The experiment is simply this. M. Bichat divided the windpipe of a living dog across without injuring the blood vessels of the neck, and had ready a plug or stop-cock, by which the passage of air to and from the lungs could at pleasure be prevented. At the same time he laid bare the crural artery of the dog, and having divided it he inserted into it a tube with a very small diameter. The coats of the artery were so pressed upon the inserted tube to prevent any blood from being discharged along the outside of the tube. He regulated the discharge of blood through the tube by a stop-cock. The diameter of the tube was so small that blood might flow through it freely for a considerable time without endangering the life of the animal. M. Bichat closed the windpipe, but allowed the passage through the tube in the artery to be open. In thirty seconds the blood, in flowing from the artery, began to darken; in a minute it was still darker; in a minute and a half or two minutes it became completely venous. Upon removing the plug from the windpipe the blood discharged from the artery gradually, and in the same periods nearly resumed the arterial colour. In this experiment it must be remembered that, when the passage of air into the lungs was prevented, the expiration of air was at the same time prevented. So that, during the interruption of respiration, the air which would have been discharged from the lungs, and which is supposed to come from the blood and to deprive it by that removal of the purple colour, is the only air which could in these circumstances be taken up by the imbibers of the lungs, and, being thus restored to the blood, would necessarily give it the colour which had been changed by its removal. Or the air in the lungs must be so compressed as to prevent any ebullition from the blood in the pulmonary arteries; and the blood would enter the pulmonary veins without being freed from the recreation, which is supposed to be discharged by that ebullition. The fate of this is that of many other physiological experiments. It discloses facts which, when considered with a fuller knowledge of the circumstances connected with the subject, became subversive of the doctrines which those experiments were designed, and for a time were believed, to establish.

tiguous in a state of cohesion. The particle, therefore, will occupy a greater space after its separation than it did before that event. Its fabric may be liquid or it may be gaseous. Whatever be its state, an increase of bulk seems to be the necessary consequence of any definite quantity of these particles. In this condition the recrementitious matter is received into the venous capillaries. Let us consider the effect which the introduction of this matter into these capillaries will have on the motion of the venous blood. The expansion of the received fluid will dilate the capillary veins until resistance to further dilatation is made by the coats of the vessels. The contained fluid will necessarily be pressed upon and will flow through the outlets that may offer. It will take the course in which it meets with the least resistance. The resistance in a retrograde direction, or that towards the extreme arteries is greater than that to which it is exposed in the direction of the heart through the venous channels, as in that direction a part of the pressure of the atmosphere is removed by the suction of the heart. A swelling tide, from an inexhaustible source, advances along the venous canals. It is by this tide that the veins are prevented from collapsing in consequence of the suction of the heart. It is the force of this tide that balances the difference of specific gravity between the blood contained in the veins and the element in which they are partly immersed. This tide gives to the veins the property of rigid tubes, so far as the

suction of the heart is concerned, and extends the influence of that suction to the remotest parts of the venous system.

The arguments that have been adduced in favour of an expansive force being, in certain instances, possessed by the venous blood, are confirmed by other proofs, and by the facility with which it explains all the observations relating to it. If the feet be immersed in warm water, the superficial veins of the feet and legs are observed to swell. This is, no doubt, occasioned by the contents of the veins being expanded by the increased temperature, and implies the existence of such a property in an inferior degree under a lower temperature.

An objection which I early saw would be made to the doctrine of the motion of the blood being occasioned by a diminution of atmospherical pressure from one end of the tube, or, in other words, by suction, was, that the sides of the veins being pliant would be collapsed by this agency, and the veins thereby rendered impermeable. In my thesis it was contended that a power sufficient to keep the veins from collapsing might be derived from the heart and arteries, the celebrated *vis a tergo*. It afterwards, however, occurred to me that the collapsing of a pliant vessel could only take place when a fluid was sucked through a medium of a less specific gravity than itself; and that as the blood vessels were immersed in a fluid of the same specific gravity with the fluid they contain, the suction of the heart or pumping would not collapse the sides of the

vessels through which the blood was drawn. As the blood vessels have everywhere some adhesion, or some external connection keeping them in the tubular form, this seemed sufficient to remove the objection. It certainly must, as I have contended, be admitted to have that effect completely as regards the motion of the blood in fishes and *fœtus in utero*; and in all those animals whose bodies are immersed wholly and constantly in a fluid of the same specific gravity with the blood; and fully explained why elastic lungs were not found in fishes, and why they were not brought into use in the *fœtus in utero*, as the simple expansive effort of the chambers of the heart was in these circumstances sufficient to pump the blood from the remotest parts of the body. But in the mammifera after birth, the bodies of which are immersed in a fluid of less specific gravity than the blood, though the blood vessels may generally be immersed in the fluids of the animal; yet the veins often not only run superficially, but are raised considerably above the surface of the body, the explanation did not seem fully to remove the objection.

Another cause was adduced which must be admitted to have considerable influence. This was the incompressibility of the capillary veins which was supposed to be a property arising from their very minute diameter, and in consequence of which these vessels were secured to be always full and permeable; and thus it was only the larger superficial veins, the permeability of which could be obstructed by the collapsing of their sides. I was still obliged to have

recourse to the exterior connexion and structure of the vessels so situate. The objection respecting the collapsing of the veins in any condition has now been obviated in the most complete and satisfactory manner. The sanguineous tide swelling from behind and springing from an inexhaustible fountain is not only sufficient to keep the vessels from collapsing, but, unless a timely vent were allowed to the current by the action of the heart, might distend the veins to an inconvenient size.

The views which I have so long supported respecting the motion of the blood were, I was convinced at their first perception, true and founded in nature ; and the objections that were urged against them, however plausible and however insurmountable they might appear to others, were never appalling in my eyes, as I was satisfied they would be removed in time by a frequently renewed consideration of them under various aspects.

The theory of the motion of the blood, which is distinct from that of the circulation, and which was in a great degree unknown to Dr. Harvey or any of his followers, as it is now matured, may, I trust, be ranked, in point of certainty, with the best established views of physical science.

It may appear that the quantity required to be constantly supplied from the source now stated supposes an expenditure or change of the materials forming the system greater than in reality can take place. The rapidity with which the constituent parts of the body are changed is, I believe, and as I have

contended in another work, much greater than is usually supposed. I believe that the process of putrefaction goes on at least as rapidly during life as it does after death; but that the effects of it are unnoticed before death, as the products of putrefaction are carried off by breathing and other processes before they become offensive to the senses. A small quantity of solid material, when converted into gas, will produce a considerable expansion of the veins; and will, therefore, in the circumstances stated, promote, in a considerable degree, the motion of the blood. But the recrement of the system is not all derived from the change of the solids. A portion of the blood which has never been located will, at every circuit, have undergone that change which unfits it for the offices of arterial blood, and will require to be removed from the system as well as the refuse of the solid parts. This portion of blood will be in the same dissolved condition with the refuse of the solids, and will be a source from which the power supposed to be generated in the extreme vessels will be supplied.

In the view that has been given of the powers generated at the terminations of the vascular system for promoting the circulation of the blood, we are struck, as in many other instances, with the economy of nature in using the same substances for different ends. The materials of which the solid or fixed parts of the system are composed, after having discharged the office that belongs to them in that situation, become dissolved in the mass of blood, and tend

greatly, by their expansion, to forward the blood to the heart and lungs. But the work of these materials is not yet finished. Having reached the lungs, and being there subjected to a diminished pressure, they assume more completely the gaseous form ; and in this manner mitigate the violence of the heat evolved by the conversion of the inspired air into a liquid before they are discharged in breathing. It may be conceived also of these materials, that, previous to their becoming fixed, they assisted in the production of animal heat and were the vehicle through which it was transmitted through the body.

An observer will not fail to be struck with the great benefit derived from the absorption of the veins, as now explained, in preventing obstructions. Not only is the collapse of the veins, which might be supposed in some cases to occur, prevented in every instance, but obstructions in the course of the veins arising from long continued pressure, from ligatures and bandages long and tightly applied, from bruises, &c., are removed. For, in consequence of the increasing dimensions in the mass of the blood in that part of the veins beyond the obstructed part, the vessels are dilated as with a syringe or blow-pipe ; adhesions which may have taken place are removed ; and the circulation restored to a healthy condition.

In the article on absorption I have endeavoured to explain why the bodies of animals were not pressed into a solid mummy-like mass by the pressure of the atmosphere, squeezing out of them all the blood and lymph with which they were filled ; on

the supposition that the capillary veins, in consequence of their extremely minute diameter, were to a certain extent incompressible, or that they preserved their tubular form in opposition to such a degree of external pressure as was greater than the difference between the specific gravity of the medium in which the tubes were immersed, and the fluid to which their open mouths had access. The supposed incompressibility of the venous capillaries will be greatly aided in the office ascribed to it; by the expansive nature of the recrementitious matter entering the capillaries with the blood; and, indeed, in case of a deficiency or nonexistence of this incompressibility, would be always sufficient to prevent the apprehended condition.

The power which has now been unfolded is of the centrifugal kind. The power, on the other hand, derived from the diminution of atmospherical pressure from the blood returning to the heart is centripedal. The first has a tendency to give divergency to the vessels in which the fluid is propelled; the second to unite branches into trunks. These powers seem at the extremities; and, at the period in which the capillary veins are formed, to have been nearly balanced. Sometimes the smaller veins diverge and form branches, which are again united into trunks. Hence it happens that the venous system is at this point replete with anastomoses. But as the heart is approached the centripedal force prevails, and anastomoses become rare. There is one part of the venous system in which the centrifugal force prevails

even to the larger vessels. It is that part of the system in which the direct influence of the suction of the heart is removed by the intervention of the liver. Here the centrifugal force is uncontrolled, and, aided by causes arising from the structure and form of the liver itself, as has been explained in another work, forms the peculiar circulation of the *venæ portarum*.

In some individuals, particularly in old people, the veins of the extremities especially rise above the surface, so much that they appear to be half separated from the body. This state of the vessels was always perplexing, and has not been satisfactorily explained in any supposition that has hitherto been made. In a recent publication, in reference to this subject, I contended that, in old people and in some young constitutions, the coats of the veins were endowed with a resilient property, or that they approached to a state of ossification. This supposition, I am compelled to acknowledge, is not supported by observation. The coats of the veins are never found in the condition supposed. A complete and highly satisfactory explication of this phenomenon is supplied by the doctrine that has now been advanced.

The field upon which we have now entered is eminently worthy of being explored. It embraces the end for which all the other operations of the system are employed. In other quarters, the mortar, the bricks, and the joinings may be formed; but here the actual mason-work of the fabric is done. In this field, and I trust in the direction pointed at, the doctrine of inflammation promises to be advan-

tageously explored. The nature of inflammation is involved in great obscurity. Of the proximate cause of this phenomenon various discordant opinions are held. Our entire ignorance of it must, I think, be admitted. Our endeavours, therefore, to prevent or remove inflammation must, in this state of our knowledge, be always empirical ; and, in the most experienced hands, cannot fail frequently to be ill-directed.

The power thus supposed to be generated at the extreme vessels, by the expansion of the recrementitious matter absorbed by the veins, appears to be essential to the motion of the blood in all animals ; but of the greatest importance to those terrestrial animals which are destitute of elastic lungs, or which are not supplied with a heart. The operations of this power would appear to be most vigorous and extensive in birds, in consequence of the high temperature always reached by the bodies of these animals. This may, in some degree, explain why the lungs of birds are not elastic. It seems least necessary in fishes on account of the weight of the medium in which these animals live ; and could not be always effectual on account of the low temperature of that medium. It is probably by this power, or by some modification of it, that the sap is circulated through the stems, leaves, and flowers of plants.

